

Letter from America: Random Walks By Young Economists

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As I write, the economics junior job market is winding down. At Princeton, we had the unusually large number of eighteen candidates come visit and present their work, we made eight offers, and have so far had five acceptances. One of the most remarkable features of the market in recent years has been the breadth of topic that currently falls within the ambit of applied economics. As often as not, the talk gives little clue as to the field with which the candidate is affiliated, so that it is necessary to ask, if only to find out about teaching possibilities. Even so, in recognition of the extent to which economists salaries are too low, in spite of their sharp growth, new assistant professors do very little teaching, one course in the first year, two and a half courses in the second year and then, to recover from that effort, a full year off in the third. Included in the bargain is a slush fund that would buy a small house in much of the US (although not in Princeton or Cambridge). (I am exaggerating only to the extent of representing the best offers as typical.) Among the topics presented on this year's job market were studies of the prison parole system in Georgia, (several) of HIV/AIDS in Africa, of child immunization in India, of the political bias of newspapers, of child soldiering, of racial profiling, of rain and leisure choices, of mosquito nets, of malaria, of treatment for leukemia, of the stages of child development, of special education, of war and democracy, of the effects of TV coverage on democracy, of bilingualism and democracy, and many others. (Among the leading departments, only Stanford's graduate students appear to be working almost exclusively on traditional topics.) Twenty years ago, there was essentially none of this. Applied theses were mostly applied price theory, using a set of generally agreed-upon (preferably "frontier") econometric methods. Issues that seem central now, like poverty, inequality, national and international health, education, the environment, and much of economic development) were left to other disciplines on the grounds that (standard) economics had no framework for analyzing such ill-defined topics.

So what is it that economics brings to malaria, child soldiering, or the consequences of parole boards? Price theory is certainly no longer our comparative advantage. It is not that it cannot be applied to a wide range of topics, as Gary Becker and others have repeatedly shown. But if current graduate students know anything of price theory, it would have had to have been self-taught, because it is no longer on the curriculum in the "best" American departments. (Except Chicago where it hangs on by a whisker, and where in a last ditch attempt to preserve it from extinction, Becker, Kevin Murphy, and Steve Levitt are running an intensive price theory summer camp for graduate students from outside of Chicago.) The advantage that economists have, if advantage it is, is their data handling skills (most social sciences are far from comfortable with millions of observations, to say the least), as well as their well-developed armory of econometric techniques. If the typical thesis of the eighties was an elaborate piece of price theory estimated by non-linear maximum likelihood on a very small number of observations, the typical thesis of today uses little or no theory, much simpler econometrics, and hundreds of thousands of observations. (The amount of computing time has remained more or less constant.) The extent to which data can effectively be substituted for theory is clearly a topic that is being actively explored, at least empirically.

In recent years, the dominant econometric method has been instrumental variables; there is much to the jibe that students no longer look for a thesis topic, but for an instrument. As instruments have become ever more baroque, and their justifications ever more strained, and in the face of deeply serious critiques, particularly by Jim Heckman, the popularity of the method seems finally to be on the wane. In its place, there is a fast growing effort to replace econometric methodology, which can be thought of as a set of ex post fixes for non-experimental data, with real experiments, which require no such fix, and whose results are thereby transparent and convincing. There is lively work in laboratory and field experiments, often to test theoretical propositions, fed and encouraged by the growing collaboration between economics and psychology. But nothing has expanded so rapidly as the Poverty Action Lab at MIT, now the Abdul Latif Jamil Poverty Action Lab (J-PAL), founded in 2003 by Esther Duflo, Abhijit Banerjee, and Sendhil Mullainathan (now at Harvard.) Born of a frustration with the inherent unreliability of econometric work, as well as by a perceived failure of the World Bank and other development agencies to seriously evaluate their project work, J-PAL runs an extensive program of randomized controlled trials (RCTs) to evaluate social programs of many kinds, focusing mostly on health and education in poor countries, but with an eclectic overall portfolio of topics. J-PAL has got off to a

flying start. It has projects in a dozen or so countries, this year it had vacancies for more than two dozen senior and junior people, and it is currently advertising a five-day course that teaches how to undertake social RCTs, at a cost of \$3,950 a head. And the use of RCTs among graduate students and young assistant professors is expanding like wildfire; one Princeton student even persuaded a Mexican city to pave a random selection of its streets.

The movement is not modest in its claims, and it has attracted a good deal of acclaim from outside the profession. Banerjee has argued that the World Bank should cease to fund *any* activity (including presumably macro policy advice) that has not been previously subject to evaluation by an appropriate RCT. Among other plaudits in the press, *The Lancet*, noted that "The World Bank is finally embracing science" (if only *The Lancet* would do the same in its treatment of economic issues!) and praised J-PAL for its role in pressing the Bank in that direction. There is much to be excited about in this program. J-PAL and other experimental researchers have come up with several surprising results that upset previous beliefs. And by replicating similar experiments in different settings they are beginning to create an impressive and valuable body of evidence. As might be expected in the first flush of enthusiasm, there has to date been less attention to some of the problems that have bedeviled RCTs in medicine, such as their limited value to physicians in practice, nor to the extent to which RCTs really do solve the standard problems of econometric analysis. (Indeed, many RCT papers subject their experimental results to various econometric corrections and analyses.) And the jury is still out on whether RCTs are any better than large data sets as substitutes for theory.

In the end, it is hard not to think that the quality of research owes more to people than to methods. Certainly, the best of the job market candidates this year made important advances and showed great imagination and skill, irrespective of the unresolved methodological debates that divide the profession. Given this abundant talent, and the new-found (or re-found) commitment of young economists to the great issues of poverty and health around the world, there is surely no fear for the future of economics. And perhaps one day soon, there will once again be a closer dialogue between theory and application.

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